

the Bureau's "Division of Scientific Inquiry," the other the hatching station, maintained by the "Division of Fish Culture." These two establishments are under different heads, and are, so far as possible, independent of one another, though they to a large extent share the same buildings. The biological laboratory, in the general scope of its work, does not differ widely from the other leading marine laboratories of the world. It is true that greater emphasis is laid upon the economic aspect of marine biology, and a number of investigators are each year employed by the Bureau to conduct researches upon such subjects as the food value and chemical composition of marine organisms, or upon problems directly related to the natural history of food fishes; but to say that investigations are restricted to such lines is to do great violence to the facts. The Bureau has always put a liberal construction upon the word "practical," realising that no hard-and-fast line should be drawn between pure and applied science, and regarding all information as ultimately useful which gives us a deeper insight into the life of the sea.

The two biological laboratories at Woods Hole—the "Marine Biological Laboratory" and that of the Bureau of Fisheries—have worked together side by side since the establishment of the former in 1888. The choice of its name by the privately supported institution has—unintentionally, of course—been the source of much of the misapprehension of which I am speaking. The name, "*The Marine Biological Laboratory*," would seem to imply an exclusive occupancy of this field, whereas the United States Fish Commission was conducting biological work at Woods Hole as early as 1871, and its present laboratory building was erected in 1885.

Another source of misconception is the fact that the Fisheries Laboratory has no definite organ of publication. Its scientific results, so far as they are economic or faunistic, or in any way related to the natural history of the sea, are in a large measure published in the *Bulletin of the Bureau of Fisheries*. The results in other fields of work are embodied in papers—and the number of these is great—scattered through all our various biological journals in this country and abroad. On the other hand there exists the "*Biological Bulletin*," which is the official organ of the Marine Biological Laboratory, though it also accepts contributions from workers in all parts of our country, oftentimes including those in the Fisheries Laboratory at Woods Hole. It is needless to add that the Marine Biological Laboratory does not make the least pretence that this journal represents its own output in any exclusive sense.

But, after all, the main source of confusion relative to the two laboratories at Woods Hole is the fact that the biologists there form a single scientific community, the members of which mingle freely together without regard to their place of work. In fact, the same investigator may work one year in one laboratory, the next year in the other, or he may even hold tables in both simultaneously; and so the reading public lumps together all our productions as Woods Hole work, and draws no fine distinctions. In the circumstances, it is natural that the laboratory which bears the name of "*The Marine Biological Laboratory*" should be frequently credited with the entire output. This is written in no spirit of resentment, but merely as an explanation of the prevalence of this widespread misconception of the situation at Woods Hole.

It will be cheerfully granted that the "Marine Biological Laboratory" accommodates a much larger number of investigators—perhaps twice as many, on the average—as does its sister institution; and it is not likely that anyone connected with either laboratory fails to recognise that a considerably greater output of scientific results must at present be credited to the former. On the other hand, the Government laboratory had, until recently, the only really efficient steam vessels available for scientific research, and has had other decided advantages in its physical equipment. But we at Woods Hole waste little time in idle comparisons such as these. Most of us are too busy endeavouring to make an occasional contribution to our common science.

Whether or not the two laboratories will continue to cover so largely the same field of activity it remains for

the future to decide. These are some good arguments for a greater division of labour than at present exists.

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U.S. Bureau of Fisheries at Woods
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Washington, D.C., October 12.

The Cocos-Keeling Atoll.

I CONSIDER myself fortunate that the author of the review of "*Coral and Atolls*," which appeared in *NATURE* of October 6, has addressed two direct questions to me, for in the answering of these questions it may be possible to open in a more frank manner the discussion of those problems with which I have dealt, and which are to be solved by dispassionate argument and investigation rather than by anonymous destructive criticism.

The first question which the reviewer puts to me is couched in the following form: he rightly asserts that I assume the lagoon of an atoll to be a slightly submerged reef, and then he asks, "Why this assumption without evidence?" For answer I would point out that the evidence is given freely in the work which he reviews (notably at pp. 251-2, and elsewhere), and, since he has apparently overlooked it, I will repeat that it consists, among other things, of the fact that submerged atoll-shaped reefs, and reefs also atoll-shaped, but of which some portion of the outer rim is awash, or on which some island débris is piled, are well-known geographical facts. The central part of the submerged reef forms the lagoon of the developed atoll, which is therefore not inaptly described as a "slightly submerged reef."

His second question is in connection with the mode of formation of atolls from the disintegration of high oceanic islands surrounded by a barrier reef. He asks me how I would explain "Agassiz's wonderful series of photographs of Fijian islands within barrier reefs" when I state that "the picture of the high island towards the completion of the process, when, after having stood resisting in a troubled sea, it so conveniently crumbles to pieces within the calm of an encircling barrier reef, appears to me to be contrary to all natural laws." I would give as explanation the very obvious suggestion that the formations illustrated (I presume in *Bull. Mus. Comp. Zool. Harv.*, vol. xxxiii.) are not the outcome of the development of the barrier reef, for similar conditions are found, quite apart from any coral structures, all over the world, the coast-lines of islands in northern seas providing equally good examples.

It may perhaps be permissible to extend this reply so as to embrace the answers to some assertions of the reviewer regarding corals and coral islands, and to point out some misquotations from the work under discussion and some misconceptions of its conclusions.

The variability of the growth forms of corals is one of the problems discussed, and I have urged that sediment—as a factor of the environment—is a potent cause of modified coral growth. That the environment as a whole, and not merely the presence of silt, was considered, may be gleaned from the discussion of the growth of young colonies of *Pocillopora* (p. 100).

The reviewer turns from this to observe that the variability of corals "may aptly be compared to the growth shown by our forest trees in different environments. Reef corals, too, resemble trees in that they are largely dependent for their food on chlorophyll, which is present in minute algæ living in their digestive cavities. The coloration of most reef corals is largely due to these algæ, and their mode of growth is sympathetic to them in that the coral skeleton is deposited so as to expose the polyps to the maximum amount of light." The reviewer then adds, "Such appear to us the ordinary views of zoologists."

The only logical meaning that I can attach to this is that zoologists as a class ordinarily believe that the variability of the growth forms of corals—and of forest trees—is due to the fact that they contain chlorophyll in their tissues. That zoologists as a class would subscribe to this thesis appears to me unlikely, and the reviewer has yet

to explain the variability of those corals which, as is well known, possess no symbiotic algae.

It would seem almost unnecessary to point out to one who criticises this work as being "in no way scientific" that the argument that, because forest trees are largely dependent on chlorophyll for their food, and corals are also largely dependent on chlorophyll for their food, therefore the reactions of forest trees and corals to varying environments will be similar, is not strictly scientific either. That the final test of specific form of corals must be the characters of the zooid is a fact on which I have insisted throughout, and the similarity of zooids in dissimilar forms is a fact that I have noted, although the reviewer appears to have entirely overlooked it.

That Wayland Vaughan found no difficulty in transplanting corals does not surprise me, for many others (myself among them) have also experienced no difficulty in the mere transplantation—any more than in the mere transplantation of "forest trees"; the difficulty only comes in (as is expressly pointed out, p. 123) when the coral is transplanted to an environment in which its growth-type, developed in a different environment, is unsuitable. In the experiments described I have pointed this fact out, and dwelt especially on cases of removing corals from a rough-water environment to a place where still water and the deposition of sediment were the prevailing conditions. In doing these experiments I was unconsciously repeating those previously carried out by Ehrenberg. Darwin (quoting from Ehrenberg's "Über die Natur," p. 49) states that "where there is much sediment placed so as to be liable to be moved by the waves, there is little or no coral: and a collection of living specimens placed by him on a sandy shore died in the course of a few days" ("Coral Reefs," p. 89).

To pass to another section of the article, the reviewer correctly says that I describe the encircling reef as "a mosaic inlay of coral fragments cemented together into a solid platform," and then adds, "but there is no evidence that it was ever really examined"! Apart from the ungenerous suggestion that in a fifteen months' investigation of the atoll the encircling reef was never really examined, and that the author drew on his imagination or on previous descriptions for what lay for ever open to his inspection, is the gross oversight that a chapter is devoted to this feature, that its structure and formation are described (pp. 163, 254, &c.), and illustrated at Plate xiii., which shows a fractured surface of the "mosaic inlay." Had the reviewer genuinely thought that the encircling reef was never examined he was over-generous in describing me as "a painstaking naturalist"; if he did not entertain this thought, then he did ill in embodying this remark in his article. It is obvious that in places the reviewer has failed to grasp the meaning of the text which he would criticise, and, in speaking of the encircling reef, he charges me with drawing deductions from "a similar platform . . . found at 13 feet above mean tide-level." I presume that this charge is based on the statement at p. 283, that "where these steps are evident, the island rise is 13 feet above mean tide-level." The "island rise" is explained (and figured at Plate xiv.) as the rise of debris piled on the breccia platform, and in the sentence immediately preceding that quoted it is definitely said that the "steps of breccia rise one above the other to a total number of three or four, and to a height of almost as many feet." What is actually found, and what the description would appear to clearly depict, is a portion of breccia platform elevated to a height of something less than 4 feet, surmounted by a pile of debris reaching to a total height of 13 feet. What ambiguity might be imputed to the text should have been removed by one glance at Plate xv., which was specially included to depict the condition. I would point out to the reviewer that had intact breccia platforms been found at 13 feet above mean tide-level more dogmatic statements concerning probable elevation might have been made.

The reviewer has not only laid himself open to the charge of having failed to survey with accuracy the material he criticises, but he has also slipped into misquotations. Concerning the base on which coral structures are built, he cites me as saying that "it matters not what the base may be so long as its platform comes within the

wind-stirred area." This area (above the limiting line of sedimentation) is, in the passage misquoted (p. 246), and consistently throughout, called the *wave-stirred* area. The two things may appear identical to the reviewer, but I think that his failure to grasp the difference diminishes the value of his criticisms upon the point.

F. WOOD-JONES.

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I HAVE not a copy of Mr. Wood-Jones's work before me, but I regret misquoting him with regard to the *wave-stirred* (not *wind-stirred*) area; I still desire more information on the limiting line of sedimentation.

Readers of NATURE must decide how much scientific evidence is given that the lagoon of an atoll is a slightly submerged reef. Perhaps I misunderstand the term "slightly submerged reef." I do not regard the lagoon in an atoll, which was formed, as Darwin suggested, by subsidence, as covering a reef at all. I do not think the borings in the lagoon at Funafuti suggest a reef such as surrounds a lagoon, and I do not consider that the nature of the rock under atoll lagoons is or can be settled without borings. Some evidence was doubtless obtained by Mr. Wood-Jones by dredgings, &c., as his book shows he has as thoroughly examined his atoll as he was able, but I do not regard the existence of atolls and atoll-shaped reefs elsewhere as more than indirect evidence of that which exists at Cocos-Keeling.

In Fiji there are many elevated islands. Some of these have fringing and others barrier reefs, which superficially appear to be of the ordinary coral-reef type. Such reefs cannot have existed when the islands were first elevated, and it seems to me that Agassiz's photographs show that high islands do crumble to pieces within the calm of encircling barrier reefs. This process would certainly be convenient for the formation of fringing and barrier reefs round these islands if organisms exist there which prevent the processes of disintegration from extending below the water-line over certain areas around them. In any case, it seems to me certain that islands within barrier reefs are being removed by some agency or other, and that the resulting reefs would simulate atolls.

So far as I can find out in the Madreporaria, the extraordinary variability in growth form has only been described in reef-builders which possess, in some form or other, chlorophyll in their tissues. I do not think my paragraph on the subject will be misunderstood by your readers. I adhere to my statement that "our author does not appear to have examined the zooids to see whether he is really dealing in any genus with one or more species," and leave this question to be settled by your readers.

Of course, the encircling reef was *really* (or genuinely) examined, and with considerable care, but I certainly did not consider, from Mr. Wood-Jones's work, that there was internal evidence that he had *sufficiently closely* examined it. I cannot follow all Mr. Wood-Jones's paragraph, but I quite fail to see where I have charged him with drawing deductions as to the encircling reef from "a similar platform found at 13 feet above mean tide-level." It would perhaps have been better if I had not used the word *similar*.

THE REVIEWER.

Early Burial Customs in Egypt.

IN his letter to NATURE of October 20 (p. 494) Prof. Flinders Petrie says:—"The whole question lies in a nutshell. Many thousand graves have been examined by one party of observers, and certain results repeatedly found. Many thousand graves have been examined by another party of observers, in other localities, and such customs have not been found."

I would analyse the contents of the nutshell in a slightly different manner. Both parties, working in every region in Egypt, have found precisely the same state of affairs. One party, under the influence of the glamour of Egypt and what is said to happen in early Italy and Europe and modern Africa, interprets it as evidence of cannibalism and "Jack-the-Ripper" practices. The other has put forward a simple record of all the facts observed and the